

CLINICAL LECTURE

ON

THE RELATION OF CLINICAL OBSERVATION TO THERAPEUTIC RESEARCH.

Delivered at King's College Hospital, London.

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GENTLEMEN,—The commencement of a summer session brings to us fresh work. It would be neither pleasant nor wholesome for us, at this season of the year, to pass much of our time in the dissecting-room or the *post mortem* theatre. These are places of winter resort. But, in the official division of our studies, there are many important subjects which claim our attention in the summer session; and amongst these is the subject of therapeutics.

Materia Medica and Therapeutics is the title of the College course; but the time which the professor has at his disposal for dealing with this large subject is so brief, as to enable him to do little more than roughly classify the materia medica into therapeutic groups. Moreover, this course comes at so early a period in your career, that you are not at that time in a position to associate theoretical teaching with any amount of practical clinical observation.

It happens quite accidentally that, by the indisposition of Dr. Lionel Beale, who, I am happy to tell you, will be able immediately to return to his work, I have to give you the first clinical lecture of this session; and I wish to turn that opportunity to the best account I can, by pointing out how the summer clinical work may, and should, be made mainly subservient to the systematic cultivation of therapeutic knowledge and the thorough testing of therapeutic theories. Sound pathology is the basis of all your medical culture; but it is as therapeuticians you go into the world, and it is as therapeuticians you claim credit of your fellow-men. The anatomist, the physiologist, the chemist, the botanist, and the pathologist, all labour in common to make you therapeuticians. It is to this end that all our teaching and all your labours are, or should be, directed. And bear this in mind, that, if you take into the world the latest and fullest knowledge of morbid anatomy and of physiology without that knowledge of therapeutics which is within your reach, you are, if you pretend to be physicians, as much shams and humbugs as the most unlearned charlatans.

I am well aware that most valuable therapeutic teaching is daily given within the wards of this and other hospitals by their physicians and surgeons; but I wish to point out how your clinical observations might, at any rate during one season of the year, be especially devoted to systematic therapeutic work. Some of you are aware how I attempted, under some difficulties, to give to the clinical teaching in our out-patient department last summer a therapeutic direction. I hope to renew the attempt this year. But out-patient practice does not lend itself well to accurate therapeutic observations. We have, in that department, to take too much on trust; we have to make large allowances for the blunders and the credulity of the ignorant; we have to be constantly on our guard against deception; and we know not what counteracting agencies may interfere with the action of our remedies when our patients are beyond our observation. Nevertheless, we do make out a good many rough therapeutic facts amongst our out-patients. We see anæmic women recover their colour and their strength from the administration of preparations of iron; and we observe that this effect is aided and hastened by the combination of aperients with the iron. We find that attacks of epilepsy are arrested or diminished in frequency by the use of bromide of potassium; and we learn that, commonly, when small doses (twenty grains) of this drug do not produce this effect, large doses (sixty grains) will. We see syphilitic nodes and painful periosteal inflammations and thickenings disappear with certainty under the influence of iodide of potassium. Recently we have seen neuralgias of the face and head of long standing remarkably relieved by the croton-chloral hydrate. We see tape-worm expelled, again and again, by the extract of male fern; and the common round intestinal worm by santonine. We see a few doses of lime-water arrest the vomiting of an irritable stomach; and a few doses of carbonate of soda and ammonia the diarrhoea of acidity and excess.

Numerous facts of this kind we can observe amongst the out-patients, but therapeutic problems of any intricacy must be resolved in our clinical wards. It has been too much the fashion—a fashion which I am will-

ing to believe is passing away—to bring a sceptical and a negligent spirit to the consideration of that part of the clinical observation of disease which is especially concerned with its *cure*. I use the word *cure* purposely, and in its widest etymological signification; because I have noticed a remarkable and foolish indisposition of late to use this word, as if it carried with it an unscientific flavour; whereas the *cure* of disease is, without doubt, the purpose of our existence as physicians. You will probably think that I am overlooking the *prevention* of disease; but, on the contrary, I am thinking of that especially, and I am marking a significant fact—viz., that those who devote themselves to the work of preventive medicine are gradually being withdrawn from the work of the practical physician. We are, at this moment, regretting the loss of a distinguished and valued colleague, who has thought the practical work of preventing disease a more attractive occupation than the practical work of curing it. And we shall, I believe, find that these two departments of practical life will become more and more distinct. The prevention of disease will become more and more exclusively a branch of applied science, and be, as a practical art, more and more separated from the work of curing disease.

Do not misunderstand me. The pursuit and progress of pathology and of therapeutics will ever be enabling us to contribute to the principles of preventive medicine; we can never dissociate ourselves from this great work, but it is to cure such disease as exists that is our *raison d'être*. Regard, then, the remedial treatment of disease not as the least, but as the greatest, part of your duty as clinical observers, and avoid a too sceptical, as well as a too credulous, spirit in estimating the influence of the remedies which are used in the treatment of disease. To gain accuracy, precision, and definiteness in the use of remedies—to enable us to teach the principles of the correct treatment of disease with the same certainty and positiveness which we apply to the teaching of the principles of diagnosis—is the end and object of therapeutic research. Could this position ever be attained, we should be enabled to reach uniformity in medical practice, and so remove the chief opprobrium that rests upon our art. My object is to point out how clinical observation may best be turned to account for the furtherance of this desirable end. And it is important to bear in mind that, whatever other methods may be made use of in therapeutic research, the clinical method must ever be the final and decisive test.

Much valuable labour has recently been expended in the investigation of the physiological action of drugs by means of experiments on the lower animals, and much that is helpful and suggestive in our clinical work has been derived therefrom; but there has been growing up a tendency, which seems to me to need reproof, to regard such a mode of investigation as the only one which should be called “scientific”; so that, while no “scientific” reputation is awarded to the most patient and astute interrogation and observation of clinical facts—a process which may call forth all the highest qualities of the mind—a limited number of experiments on the lower animals, the true value of which I shall immediately attempt to estimate, is sufficient to procure a reputation as an original investigator. Let us, by all means, and with all heartiness, give honour to real work of all kinds; but do not let us fall into the serious mistake of estimating the scientific value of an investigation by its remoteness from practical utility. The observation of the action of medicines on the lower animals is a considerable aid to clinical work, but it serves rather as affording hints as to the lines of investigation which should be followed by the clinical physician, than as giving him definite knowledge upon which he can safely act. Unfortunately, most of the animals upon which we are able to perform an experiment are very differently affected by certain drugs from what human beings are. Belladonna, stramonium, and hyoscyamus, for example, potent drugs when admitted into the human body, may be eaten by rabbits with impunity. Goats seem insensible to the action of tobacco, which is, as you know, a fatal poison to man when it is taken into his system even in small quantity. Dogs are singularly insensible to the action of aloes; two or three grains will produce a purgative effect on most of our patients; it takes fifty or sixty grains to act in the same way on a dog. On the other hand, half the amount of our ordinary doses of calomel will produce serious disturbance in the constitution of that animal. Again, opium, and most other narcotics, exert less soporific influence on the lower animals than on man. In the experiments I have recently been making with croton-chloral, the difference in its effect on the dog and the cat were quite remarkable; from fifteen to twenty grains proved a fatal dose to cats, while sixty grains merely sent a dog to sleep for less than two hours. Tartar emetic has scarcely any physiological effect on horses and cattle, but a few grains cause immediate vomiting in a dog. These and other facts of a like kind teach us to be exceedingly cautious in our inferences as to the action of medicines on the human subject from the result of experiments on the lower animals. We may gain, in this manner, approximate information as to dose, and in many instances

we may obtain much valuable information as to the physiological action of drugs; but the test of cautious clinical observation is in all cases the essential and corrective complement, as well as the final appeal from all such physiological investigations.

We come, then, to this conclusion, that, although physiological investigation may be a most useful auxiliary, yet the only sound basis of therapeutic research is clinical observation. The clinical physician must not undervalue the work of the physiologist; the physiologist, when he works at therapeutics, must not think himself independent of the clinical physician. The future progress of therapeutics depends upon the union and co-ordination of the two methods.

But we are especially concerned now with this consideration: How can we best turn our clinical observation to the advancement of our therapeutic knowledge? In the first place, we must take a real interest in the treatment of disease. It may seem almost unnecessary to premise this; yet it is unquestionably true that, in hospital practice, the treatment of disease occupies a very secondary place in many of your minds. This is, I am glad to admit, less evident than it was a few years ago; but one of the sins of that time, for the existence of which I hold the so-called expectant method mainly responsible, was a prevailing indifference amongst students of medicine as to the treatment of disease. The natural history of a disease, its careful diagnosis, its morbid anatomy when fatal, were subjects to which your attention was fitly and eagerly devoted; but the idea of attempting to arrest or modify its progress by remedial measures seemed to have but little hold on your minds. The influence of examinations, coupled with the absence of agreement and uniformity in the treatment of disease amongst clinical professors, contributed to this neglect. Most students live with the fear of the examiners constantly before their eyes. In studying the diagnosis and morbid anatomy of diseases, they knew they were on pretty safe ground; but when they were aware that it was a matter of uncertainty whether they met an examiner who would expect them to treat rheumatic fever with alkalies, or lemon-juice, or with blisters, or with hellebore, or with quinine and opium, or with mint-water, it was perhaps not without a touch of practical wisdom that they reserved the consideration of the treatment of disease until the ordeal of examination was overpast; they knew that a little reticence, a little hesitation, a little modest deference to the superior experience of the examiner on this point, was a safe attitude to assume.

You must, then, in the first place, take a real interest in the treatment of disease. Watch the influence of a dose of medicine with the same attention and earnestness as you note the rise and fall of temperature, the situation of a cardiac murmur or a friction-sound, the composition of the urine, or the structural lesions which the *post mortem* room reveals. You will have to do so, if you wish to become a successful practitioner when you pass into private practice; and you should begin to do so in your hospital work; and although I have spoken of these as "little" things, it is by their means that disease, in many constitutions, is kept at bay. Private practice is, to a great extent, made up of little therapeutic refinements, little aids to struggling function, little attentions to individual peculiarities, which it is difficult to teach you in the wards of a hospital; but we might teach you a great deal more of this most helpful knowledge, if you had the desire and the patience to learn.

In the next place, let your clinical observation of the effect of remedies be systematic. Let me point out what your system should be. It is of prime importance, in attempting to draw any valid conclusion from the clinical observation of the effects of a remedy, that such observation should extend to a great number of similar instances. We must always be on our guard, lest we fall into the popular fallacy of judging hastily from results, or arrive at rapid conclusions from a single or only a few observations. If we act so, we shall do what the public are for ever doing with regard to medical practice, *i.e.*, mistaking coincidences for consequences. Besides, it is the great misfortune of our art, to be invariably exposed to those disturbing conditions which logicians treat of as "the plurality of causes," and "the intermixture of effects"; and, since we can scarcely ever get rid of these embarrassing circumstances, we must do what we can to neutralise their effect by the numerical strength of opportune instances. One of the disturbing causes of this kind, which we can never eliminate entirely, is the influence of the mind over the body. It may occur to you, that there is not much "mind" about many of our hospital patients to exercise this disturbing effect, and I am inclined to agree with you; but the little there is generally of a very superstitious quality, easily imposed upon by others, prone to impose upon itself.

You know very well that we keep two very harmless mixtures in the hospital; one we call *mistura flava*, and the other *mistura cerulea*; they are both, I need not say, purposely made quite inert. I have pointed out, again and again, cases amongst the out-patients who, week

after week, have made us weary with tedious reiterations of their numerous indefinite symptoms; and when, for some time, we have made many well-intentioned, but fruitless, efforts to relieve, at last we have, in despair, had recourse to our *mistura flava*, or *mistura cerulea*, and then, to our surprise and amusement, they have returned, praising our last prescription as the "only thing that had done them any good". Hence I have always been in the habit of recommending you, in dealing with the class of patients we see in hospitals, to follow this brief rule, "Believe what you see, doubt what you are told". I do not mean by this, that you are never to treat subjective states; on the contrary, they require careful management, and will often demand your greatest skill and discrimination; but deal with them always as subjective states, and do not associate them with anything objective, save as the result of your own physical observation, or previous experience.

I cannot too strongly insist on this simple rule in dealing with the ignorant and superstitious; for, not only are they incapable of correctly analysing and describing their own sensations, but, by a mixture of incapacity and exaggeration, their statements will often tend wholly to mislead you; while the *post hoc ergo propter hoc* argument is ever present with them.

You must have seen, many times, how most of the cases of so-called heart-disease amongst the out-patients have turned out, on investigation, to depend on a flatulent stomach; while we are often led to the discovery of real heart-disease, not from any complaint of that organ by the patient, but from other indications, which are perceived by our own senses. We constantly hear patients complain of pain in the "kidneys", or often, most definitely, of the "right" or "left kidney". I have never known such a complaint to be really associated with disease in these organs; but we constantly detect disease in the kidneys, when it is quite unsuspected, from what we see in looking at the patient's face.

A perfectly inert fluid will, as I have said, obtain the credit of curing a patient; but the same fluid will also, by some, be accused of all kinds of ill effects. You will often be told that such a mixture has purged the patient, or caused instant vomiting, or fearful headaches, or made them "tremble all over", or produced various other less definite effects. I have had two patients come into the room, one immediately after the other, both taking the same medicine; and one has declared that the medicine purged her so that she could not take it; the other that she could not take it, it was so constipative; and this of a medicine that possessed neither aperient nor astringent properties. With the large experience of human nature which falls to the lot of every hospital physician, he can never be at a loss to account for all the marvellous evidence which homeopathy, or any other fallacious system of medicine, can produce in its favour. The first step we have to take in the direction of a sound and truthful therapeutic system is to reject evidence of this kind.

The effusive gratitude of a patient relieved from suffering, or the disappointment of another whose pain is unassuaged, tends to make them assume, almost invariably, the attitude of advocates; we, if we have a pure and single regard for the advancement of our science, must maintain the rigid impartiality, the unbiassed reason, of the judge. Therefore, I repeat, "Believe what you see, doubt what you are told". Nor must you conclude hastily from results. If it were safe to do so, therapeutics would be the easiest, instead of the most difficult, branch of medical science. How, then, is it possible to arrive at any definite conclusion in therapeutics? How can we eliminate all the disturbing influences which complicate, or counteract, or intensify, or obscure, the action of our therapeutic agents? Clinically, in two ways: first, by observing a great number of similar cases under varying circumstances; secondly, by a judicious selection of appropriate ones. The second method is as useful and more convenient than the first, but the type of patient we require for this purpose is often difficult to find. We need a calm, intelligent, observant, cautious, and, above all, a sceptical (in the best sense) person, free from habits of self-regard, and not over sensitive; we may learn more from the careful and trustworthy analysis of the symptoms of a disease, and the effects of our remedies, in one such person, than from a hundred average patients. But the first method is a very important one, and it is that to which, in institutions like this, we must chiefly trust.

The opportunity which large hospitals afford for testing the efficacy of particular modes of treatment, on a great number of similar cases, is one of the advantages attending their existence. Yet I would venture to hint, that we do not turn this opportunity to sufficient account. It is necessary, for many reasons, that the care of the patients in a large hospital should be divided amongst a certain number of physicians and surgeons, and it is also necessary that these medical officers should exercise perfectly independent action in the treatment of their cases; but I am disposed to think that, while we estimate highly this power of

independent action, we undervalue the benefits which would arise from a greater amount of common action, more *solidarité*, more frequent consultations and discussions amongst ourselves, as to the treatment of special kinds of disease. I take it that one of the objects of institutions like these should be, to teach uniformity of practice; and, in order to teach uniformity of practice, we must first have reached a considerable amount of agreement amongst ourselves. Please understand that I am not speaking of this or any particular hospital, but of large hospitals in general.

Now, it cannot be to the advantage of a student that he should see, except as an experimental observation, the same disease treated by two of his teachers according to opposite methods. It is monstrous to say that the student is to judge for himself. A pupil is not in a position to act as judge, in a question which is often one of considerable difficulty and delicacy, and for the solution of which a certain maturity of experience is essential. If, for example, rheumatic fever be best treated with small quantities of inert aromatic water, we can have no need of alkalies, or acids, or opium, or quinine, or the cold pack, in such cases. The true scientific treatment of a disease, if, as I believe, such a thing exists, must be uniform in principle, so far as the disease itself is concerned, though the variable constitutions of patients will require corresponding variations in the manner of applying it.

I contend, then, that clinical observation is rendered less fruitful in the direction of therapeutic progress than it might be, by the neglect of united deliberation, and by the absence of common agreement amongst the officers of large hospitals, as to the treatment of disease. Moreover, this adherence to independent action neutralises much of the advantage which might be gained to therapeutics, by the observation of the action of any particular remedy or mode of treatment, in a large number of similar instances, and for many purposes practically converts a large hospital into a number of small ones. But in testing clinically the use of a remedy, you must give it not only in a number of like instances, but also, if possible, in cases which vary in recognisable modes or degrees. In this way, you gain both positive and negative evidence of its value, and you are enabled to limit and define its applicability.

Let us take, as an instructive example, the clinical observations we made in this hospital as to the therapeutic use of the croton-chloral hydrate. It came to us with the reputation of being a remedy for neuralgia. We tested its value in a great number of cases. First, we gave it in a number of similar instances—instances which all agreed in this particular, that the nerve affected was a branch of the nervus trigeminus, and that the affection, so far as we were able to discover, was neither rheumatic nor hysterical. I am not aware that it failed to give relief in any of these instances. We then gave it in many cases where the nerve-pain complained of was clearly of rheumatic or hysterical origin; and, in these cases, I am not aware that it gave marked relief in any. But let me remark, *en passant*, that you must not be disappointed to find every remedy fail, when applied to cases in which there exists a hysterical element. Then we gave it in neuralgias occurring in any nerve—the lumbar nerves, the sciatic nerves, the intercostal nerves, etc.—and we found that, in nearly every case, it afforded relief, in many cases very marked relief; but its success was not nearly so complete, or so uniform, as in the cases of trigeminal neuralgia. A case, in which I have recently given it, is, perhaps, worth mentioning. A pale, anæmic man, a tailor, came as an out-patient, on April 6th, complaining of severe shooting pains, extending over the top of the head and the upper part of his face. These had tormented him for five years, and made his life miserable. He stated he had been under all kinds of medical treatment; he had been an out-patient at the German Hospital for ten months, and an in-patient in St. Bartholomew's for two months, but had received no relief. We gave him three grains of croton-chloral twice a day. After taking it for a week, he stated he was much better, and after a fortnight that he was almost well, certainly much better than he had been any time during the last five years. As he was very anæmic, I ordered him some iron, and to leave off the croton-chloral. The last time I saw him, the pains had returned with the discontinuance of the remedy, an useful corroboration of our inference that the relief of his pain was a consequence of the use of the drug. He has now passed into the hands of Dr. Rutherford.

But I ought to mention that this numerical method, as it has been called, is far more useful when applied to the estimation of the influence of remedies in chronic cases than in acute affections; for, in acute diseases, and especially in epidemic diseases, your observations must extend over an exceedingly vast number of instances, before your inferences as to the value or influence of any particular mode of treatment can be relied upon. The reason of this is, that these diseases present themselves at different times, and in different instances, under varying types and degrees of intensity. You will find, for example, that scarlet fever, during many years, appears in

epidemics of little severity, which give rise to little alarm. Then it suddenly appears in a terribly alarming and malignant form. Trousseau mentions a striking instance of this. He tells us that Bretonneau, that great French physician, who was Trousseau's master, taught, for nearly a quarter of a century, that scarlet fever was always a mild affection—the mildest of all the exanthemata, and that he had never met with a fatal case. But, in 1824, an epidemic broke out in Tours where Bretonneau practised, and several persons died of the fever with frightful rapidity. At first, he blamed the treatment adopted by his colleagues; soon, however, he himself lost several patients, and he at last learned to regard it as "equally mortal with plague, typhus, and cholera". I believe that cases of pneumonia may vary in type and intensity, in much the same way; so that we should be especially cautious in applying the numerical method to the study of the therapeutics of acute diseases. We need a much larger number of instances than in chronic cases, and it is necessary that they should be spread over a considerably longer period of time.

Another most important use we should make of clinical observation, in connection with therapeutic research, is in the estimation of the proper doses of medicines; and we should associate this with another consideration, and that is, the influence of constitutional peculiarities of temperament in modifying the action of drugs. I think few of us are aware how wide should be the range of dose of certain drugs, in order to affect similarly patients of opposite temperaments. I am inclined to believe that there are human beings, who differ almost as widely in the influence of medicines upon them, as my cats and dogs did in the action of croton-chloral. And this circumstance leads us to do injustice to our remedies; we blame them when they are not in fault. We constantly hear drugs spoken of as very uncertain in their action. This is an incorrect manner of expressing what is, no doubt, a correct observation. Supposing the drug to be the same in quality, it is difficult to believe that its physical and chemical properties, by which alone it can produce its characteristic effects upon our organism, change from time to time. It is more probable that the difference, the uncertainty, lies in the peculiar, and it may be inherited, qualities of the tissues in the different persons to whom it is administered. In certain persons, certain tissues are of extreme irritability; in others, they are equally insensible. We see this daily, with regard to the skin and mucous membranes. There are persons whose intestinal mucous membrane is so sensitive, that the residue of the digestion of ordinary food is sufficiently irritating to maintain a chronic diarrhoea; and it is only by adhering to a diet which leaves scarcely any solid residue that this tendency can be kept under. In our application of counterirritants to the skin, we constantly meet with the same kind of sensitiveness. I have, at this moment, a patient under my care, who attempted to use a mild mercurial inunction—an ointment one-fourth the strength of that of our Pharmacopoeia, and it produced an intense dermatitis.

You must have frequently seen most distressing iodism produced by a single small dose of iodide of potassium. You have probably seen others salivated by a small dose of calomel. I have a patient who has again and again, with the best will, tried to take quinine as a tonic, in half-grain doses, but with the invariable result of producing a severe headache in a few hours. I have another patient, in whom a dose of opium or morphia produces an intense, vivid wakefulness, for eighteen or twenty-four hours before it has any soporific effect; and when it was necessary to use morphia suppositories in this case, in order to relieve distressing nocturnal irritability of the bladder, they had to be introduced early in the morning, and then the soothing effect came on soon after bedtime.

Now, I believe we shall find the much neglected question of temperament to be at the bottom of most of these peculiarities, and I do not think it impracticable to turn our clinical observations to account in identifying and systematising such tendencies. It is, on the other hand, quite as important to detect that insensibility to the action of drugs which characterises others, and which is so disquieting and discouraging to us. I have already alluded to the hysterical constitution as especially unfavourable to the correct estimation of the action of remedies. There is a perversity about such cases, which sometimes looks like the evidence of a vicious pleasure in disappointing our calculations. We must remember, therefore, in treating any particular case of illness, that we have two things to keep in view—one the disease itself, the other the individual constitution of the patient. Sometimes one and sometimes the other will demand our chief consideration. But it is of consequence to you to know that it is not safe to conclude, from the want of success in the use of a particular remedy in the cases of hospital patients, that it will be equally inefficacious in all other instances. There can be no doubt that we see the most confirmed and hopeless cases in the wards of our hospitals—constitutions rendered callous by ill-usage and exposure of every kind; and moreover, owing to the abuse

of our out-patient system, the poor, as I have remarked elsewhere, become such inveterate drug-consumers, that, in course of time, it becomes difficult to produce any effect upon them with ordinary doses of medicine.

Another therapeutic use to which you can turn your clinical observation is to learn the best means of relieving distressing or dangerous symptoms. A distinguished clinical teacher of this town has denounced the treatment of symptoms, and says we should always try to "get behind the symptoms". Now, I must say this sounds to me like very needless advice and unnecessary denunciation; for I suppose the most ignorant practitioner who ever attended a case of illness tried, according to his light, to discover the causes of the symptoms he treated. It is one of the strongest and commonest tendencies of the human mind to try to "get behind" phenomena, and out of this tendency spring all the absurd theories and hypotheses with regard to the nature and treatment of disease that have ever existed; and bad practice does not lie in dealing with phenomena merely as phenomena, but in assuming and treating some cause of the phenomena which is not a *vera causa*. When we give ether and ammonia to relieve the dyspnoea of a weak heart, we are treating a symptom; and when we give iron and quinine and supporting food in the same case, I suppose we may be said to have got "behind the symptom". But I venture to think the first treatment is as important as the second. I know physicians who carry this avoidance of the treatment of symptoms to the extent of objecting to relieve the cough of phthisis by the use of sedatives. During the last three years, I have attended considerably more than one thousand cases of phthisis, and I have treated them with and without sedatives; and, were I unfortunate enough to be afflicted with that malady, and to have a physician who would not treat my cough, I should do my best to exchange him for one who would.

There is a symptom which we have not yet "got behind"; but we are all, I believe, agreed as to the importance of treating a symptom common to many acute diseases. I allude to the rise of temperature—the pyrexia in such maladies as typhoid and rheumatic fevers and pyæmia. Clinical observation has been latterly turned to excellent account in this particular direction. The influence of large doses of quinine in reducing the temperature of the body in pyrexia requires additional clinical examination; and it would appear that this investigation is likely to offer an appropriate example of the advantage which may accrue from the combination of such purely experimental observations as those of Binz and Geltowsky with the systematic trials of the clinical physician. Of the success of another method of treating the hyperpyrexia of certain fevers—I mean the application of the cold pack—I take it, there can be no doubt. I place before you a temperature-chart of a case of rheumatic hyperpyrexia which was treated in this manner in this hospital, and reported in 1872 by Dr. Kelly; and we have had other examples since in which this treatment of a symptom has undoubtedly been the means of saving life. And this leads me to speak, finally, of the importance and necessity of clinical experiment in therapeutic research. Experiment at the bedside has always been regarded popularly with distaste; and it is only in the wards of a hospital, where we are less fettered than elsewhere by popular prejudice, that we can pursue systematically the experimental method; and yet it is the only method from which we can hope to gain any positive advance in the therapeutic management of disease. All science rests on observation and experiment. All our therapeutic knowledge rests ultimately on an experimental basis. Empiricism, which rules the medical art, teaches that all our knowledge is derived exclusively from experience; and this experience is only the result of the accumulated observation of endless experiments. But, although medical practice is empirical, yet modern medical practice is also undoubtedly scientific. I cannot understand how those who loudly declare that all sound medical practice is purely empirical, and not at all scientific, can overlook the obvious reflection that half a century or even a century ago medical practice had very nearly as great an amount of empirical knowledge to rest upon as it has now. It had the accumulated experience of ages for its foundation and support. If this be the all-sufficient basis of practical medicine, why was the practice of that age so much at fault? Why has there been such a reversal of methods of treatment within the last fifty years? Because, I answer, of the growth of scientific knowledge, and because, therefore, the modern practice of medicine rests on a scientific as well as an empirical basis.

But it is to experiment, and experiment only, that we can trust in our application of clinical observations to therapeutic research. These experiments may be of two kinds. In the first place, we may already be in possession of one or more methods of treating a certain disease, which may be on the whole satisfactory, as, for instance, in the case of ague or dysentery; but we may desire to find out which of these methods is the best, or we may have grounds for believing that a better

method is discoverable. In such a case we have fair ground for cautious and careful experiment; but we should not be justified in making trial of anything hastily in such cases, because we have no great reason to be dissatisfied with our present methods. In the second place, we may have to deal with diseases for which we have no method of cure, which steadily tend, with few exceptions, to a fatal termination. We may take, as examples, cancer and other malignant growths, phthisis, and pyæmia. In these cases, an amount of experiment is justifiable—nay, is imperative—which would be wholly improper in the first class of cases; for in these diseases, whatever be the result of our tentative efforts, the patient cannot be left in a worse case than that in which we found him. Considerations like these justified the apparently rash experiment of cold affusion in certain cases of hyperpyrexia. It was known that cases, in which the bodily temperature reached a certain intensity, invariably ended fatally. Whatever might be the result of the experiment, it could not be worse than this; but it happened to prove successful, and we should not now consider ourselves justified in allowing any similar case to pass out of our hands without making this last effort to save life. But it would have required a very unusual amount of courage to make this experiment for the first time in private practice. Hence arises a great benefit to the public from the existence of institutions like these, where our practice is not embarrassed by personal or private considerations, but is directed solely by the desire to further the rational progress of the medical art.

There are many other important points in connexion with the application of clinical observation to therapeutic research to which I should like to allude; but I must not detain you longer now, and I shall take some other opportunity of returning to this subject. I have only to recommend you to let the therapeutic treatment of disease command your earnest attention in the clinical wards of this hospital during the session we are just commencing.

SURGICAL MEMORANDA.

SPONTANEOUS PASSAGE OF LARGE CALCULUS FROM THE BLADDER OF A FEMALE.

A SIMILAR case to that reported by Mr. Stephen Clogg in the *JOURNAL* of May 2nd was recorded by Sir Astley Cooper in *Guy's Hospital Reports* for 1838, Series I, vol. iii. The patient was under the care of Mr. Harris of Redruth, in Cornwall. She was only eighteen years old. The calculus weighed 651 grains, and measured $2\frac{3}{4} \times 1\frac{3}{4} \times \frac{1}{2}$ inches. The symptoms had existed for seven years.

RICHARD RENDLE, Guy's Hospital.

THE USE OF HOLT'S WINGED CATHETER.

I FEEL that some acknowledgment is justly due to Mr. Holt for his introduction of the "winged catheter", which, I am confident, will be found to be a very great convenience to the surgeon who has charge of a case requiring the constant use of a catheter, as well as a very great comfort to the patient. I have at present in my Infirmary a patient suffering from paraplegia caused by a cart falling on him and fracturing some of the dorsal vertebræ. When raised after the fall, it was found that he had lost all power of motion and sensation of the lower part of the body; and he has never since been able to pass urine naturally, or to retain feces. Shortly after the accident, he was visited by Dr. Barbor of Carlingford, who drew off the urine, and continued to do so as often as was necessary, until the patient was removed to the Infirmary, about October 31st, three or four days after the injury. For a few days after his admission, I used the ordinary elastic catheter; but, finding some difficulty in its introduction, and the urethra becoming irritable, I had recourse to one of Holt's winged catheters of full size with a stilet, leaving it in the bladder. I did not find the least difficulty in its introduction; on the contrary, I think I never passed a catheter with so much ease. The patient never experienced the least annoyance from its presence, and the catheter retained itself in the bladder without any of the usual fastenings. At the end of five days, I withdrew it with great ease, and found it was not at all corroded, nor was there the least concretion upon it.

My object in reporting this case is to draw the attention of surgeons of hospitals, poor-houses, etc., to the use of this instrument in all cases requiring the retention of a catheter in the bladder, as it will save the surgeon a great deal of trouble and much anxiety, and at the same time will afford the patient great comfort.

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